

as the period of the solar semi-diurnal tide, and that the solar tide might undergo such kinetic augmentation as to rupture the planet. A piece torn off might form the moon. The suggestion was only thrown out tentatively, and it might perhaps have been better had it been suppressed. The whole essence of the suggestion lies, however, in the approximate identity of the free and forced periods of oscillation, and this reasoning has no place in Spiller's theory.

In considering the history of a cooling planet, the author is opposed to Sir William Thomson, and concludes that the surface would harden into a crust. It seems to me that the time is hardly ripe for a very confident opinion on the point.

A large place is given in this book to the influence of tides in the evolution of a planet. A description is given of the tidal retardation of planetary rotation and the recession of the satellite; and the chapter is in fact principally a *résumé* of my own papers. The author is at one with me in rejecting Prof. Ball's view, that an enormous exaggeration of marine tides can have taken place within geological history. He is inclined to adopt the view that the trends have been imparted to our great continents by means of the wrinkling consequent on tidal friction in a primitively viscous mass; but he hardly notes, as I pointed out, that if this be so we have to accept a continuous adjustment of the general ellipticity of the earth to a figure of equilibrium, without obliteration of the wrinkles. The suggestion is thus perhaps placed in almost too favourable a light.

On p. 282 Mr. Winchell speaks as though solar tidal friction is adequate to cause a sensible lengthening of the year, so that in earlier ages it was sensibly shorter. It is impossible to admit the correctness of this view, as I have elsewhere shown.<sup>1</sup>

In a section on orogenic forces we have, amongst much other interesting matter, an account of M. Favre's experiment, in which a layer of clay is placed on a tense elastic membrane, which is then allowed to contract: an illustration of many of the facts of mountain geology is thus furnished.

In the following chapter the author follows the various lines of argument by which limits are placed on the age of a planet, and by a subsequent geological discussion endeavours to derive a time scale; but I feel incompetent to judge of the worth of the conclusion. We may regret to find the revival in this place of Prof. Haughton's argument, viz. that the absence of a measurable nutation of 306 days proves the enormous antiquity of the elevation of Europe and Asia. The argument is, I think, worthless, as I believe that Prof. Haughton now admits.<sup>2</sup>

The principal topics dealt with in the rest of the book are the geology of the moon, the physical condition and habitability of other planets, and the final effects of tidal friction.

The fourth main division of the book is historical, and contains a review of the evolution of cosmogonic theories, with an exposition of the speculations of Kepler, Descartes,

Leibnitz, Swedenborg, Kant, Lambert, William Herschel, and Laplace.

From the account which has now been given of this work it must be evident that Mr. Winchell set before himself a task of portentous magnitude, and that he has performed it conscientiously. The criticisms which have been made should not impair the conviction that the student of this group of subjects will find his work of great value.

G. H. DARWIN

### LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

#### The Pentacrinoid Stage of *Antedon rosaceus*

IN compliance with Prof. Herdman's request, I have to state that my experience—acquired during seven years of consecutive dredging in Lamlash Bay (1855-61)—is in entire accordance with his own. Although the most active period of reproduction in *Antedon rosaceus* is undoubtedly (as stated by Sir Wyville Thomson) the early part of the summer, so that the Pentacrinoids which spring from the ova then matured and fertilised are ready to drop off their stems in the succeeding autumn, yet I never failed to obtain *Pentacrinoids* in all stages, as well as *Antedons* still “in fruit,” throughout the months of August and September. In fact, the whole of my study of this type—which, as regards the skeleton, is fully recorded in my memoir in the *Philosophical Transactions* for 1865, and of which, as regards the soft parts, a general account is given in the *Proceedings* of the Royal Society for 1876, was carried out during those months; my official duties keeping me in London until after the first week in August.

I may take this opportunity of directing the attention of those interested in Crinoid structure (1) to a communication I have recently made to the Royal Society (*Proceedings*, May 29) on the Nervous System of the Crinoids; (2) to a paper by Prof. A. Milnes Marshall in the *Quarterly Journal of Microscopical Science* for July last; and (3) to a paper by Dr. Carl Jickeli of Jena, in the *Zool. Anzeiger*, 7 Jahrgang, No. 170.—The doctrine I propounded on this subject nearly twenty years ago (that the quinquelocular organ contained in the centro-dorsal basin of *Antedon* is a nerve-centre, and that the radial cords issuing from it, which traverse the calcareous segments of the arms and pinules, and give off branches to the successive pairs of muscles, are nerve-trunks), though supported by the experimental evidence which I published in 1876, and by the careful microscopic investigations of my son, Dr. P. Herbert Carpenter, has not been accepted by Zoologists generally; being for the most part either ignored altogether, or pooh-poohed as “evidently” fallacious, because inconsistent with homological theory. When I made my recent communication (1) to the Royal Society, summing up the very remarkable confirmatory evidence afforded by my son's inquiries, and referring (as Prof. Marshall had kindly enabled me to do) to the then unpublished results of his experiments (2), which entirely tallied with my own, Prof. Huxley, while admitting the strength of my case, remarked that the position I assign to the nervous system of the *Crinoidea* is as anomalous (in relation to that of Echinoderms generally) as it would be for a Vertebrate animal to have its spinal cord lying along its ventral surface. In reply, I asked, “What more proof can you ask for, of the nervous function of the quinquelocular organ and radial cords?” The only additional evidence that Prof. Huxley could suggest, was the result of electric stimulation. Before my paper was published in the *Proceedings*, I learnt (3) that this experiment had been actually tried four years ago by Dr. Jickeli, whose results entirely confirmed my doctrine.

It is to be hoped, therefore, that those who have so confidently and persistently clung to a homology, which is in direct contradiction to the most complete and conclusive proof that experiment can afford—supported as this is by the large body of

<sup>1</sup> *Phil. Trans.* Part 2, 1881, p. 524: “From this it follows that, if the whole of the momentum of Jupiter and his satellites were destroyed by solar tidal friction, the mean distance of Jupiter from the sun would only be increased by 1/25000th (misprinted 1/2500th) part. The effect of the destruction of the internal momentum of any other system would be very much less.”

<sup>2</sup> See *Proc. R.S.* February 19, 1878, No. 186, p. 1, “On Prof. Haughton's Estimate of Geological Time.”

anatomical and histological evidence summarised in my recent paper—will now see that unless they can disprove the statements of Prof. Marshall, Dr. Jickeli, Dr. P. Herbert Carpenter, and myself, they are bound to admit my doctrine, and to show how their theoretical homology is to be reconciled with it.

WILLIAM B. CARPENTER

56, Regent's Park Road, London, N.W., November 3

### Natural Science for Schools

THE thoughtful and suggestive paper of Prof. Armstrong in the last number of *NATURE* (p. 19) is to be commended to the attention both of science teachers and of the head masters of our schools. It is undoubtedly true that, with few exceptions, science is still either completely neglected by our schools or handled in a way which does not at all tend to advance its interests. When it is made a "refuge for the destitute," or considered only fit for those intellectually unequal to the study of classics and mathematics, no wonder that observant head masters conclude that little good is to be got from it.

As a science master of many years' experience (having been in fact responsible for the introduction of science into two of the schools named by Prof. Armstrong as exceptions to the universal indifference), you will perhaps allow me to call attention to the importance of Prof. Armstrong's paper, and to give the conclusions to which my own experience has led me.

The importance of clearly understanding the purpose with which science is to be studied, and the distinction to be borne in mind between the best curriculum for those who are to be professed chemists and those who will not carry the study of chemistry beyond their school-days is obvious; but I wish to point out how entirely science masters are at the mercy of examiners, both of University examiners, periodically examining a school, and of examiners for open scholarships. My own experience is to the point. Fully persuaded of the uselessness of attempting to make an analytical machine out of the ordinary school-boy giving two or three hours a week to chemistry for two or three years, and of the very small amount of education to be obtained from such a course, I endeavoured to model my instruction in practical chemistry much upon the lines adopted by Prof. Armstrong, and exemplified in the appendix to his paper. When the examinations came, it was duly explained to the examiner that the course of instruction adopted had been unusual, but, all the same, the papers set were of the usual kind:—"Analyse the mixture A," "Determine the metals and acids present in the solution B," &c. On such a paper, of course, the boys failed, and a depreciatory report was sent up by the examiners, with the result that the governors of the school thought it their duty to interfere, and request that "more attention should be given to practical chemistry." Consequently my attempt had to be abandoned, and we returned to our "test-tubing."

Scholarship examinations, being presumably of those who will carry the study much further, may more reasonably demand a knowledge of the ordinary methods of analysis, but I am glad to see that a considerable change has taken place in the papers set, and that now the questions proposed are often such as to place the mechanical analyst at a disadvantage, and to encourage the intelligent observation and interpretation of phenomena.

Prof. Armstrong of course writes as a chemist. But there can be no doubt that certain portions of physics are educationally more useful, and it seems to be only the difficulty of arranging practical work in physics which has led to the present state of things, where practical science work in schools means nearly always practical chemistry. But Prof. Armstrong's protest against allowing this to degenerate into "test-tubing" should not be disregarded. There seems also no reason why *elementary* instruction in science—whether chemistry, or botany, or physiology—should not deal *first* with the familiar things of everyday life. I think much more training is to be got by determining, as Prof. Armstrong suggests, the composition of air, the relative combining weights of silver and lead, &c., than by seeing made any number of oxides of nitrogen, and listening to a description of their properties. There is, however, considerable difficulty in arranging easy methods of determining chemical equivalents which, in inexperienced hands, shall give results not *too wide* of the mark.

If a boy gets out the atomic weight of oxygen as 9 when the book says it is 16, or finds the latent heat of steam to be 300 and

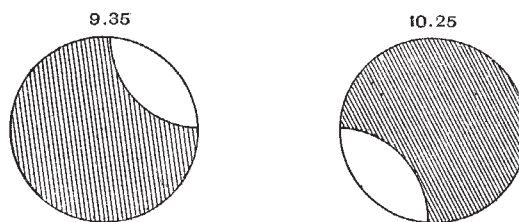
something when it ought to be 536, he begins to disbelieve in the precision of the statements made, and it is unfortunately impossible for a beginner to make *accurate* determinations of combining weights. Less erratic results can, in fact, be obtained in certain selected physical measurements.

The "bareness" of printed instructions is, as Prof. Armstrong remarks, a distinct advantage to the good student, by compelling him to think for himself, but it is fatal to the unintelligent student, to whom "thinking" is the very hardest work he is called upon to do.

SCIENCE MASTER

### The Recent Lunar Eclipse

My object in writing is to confirm in some degree the peculiar appearance of the disk, noticed in your last number (vol. xxx. p. 632). The eclipse was seen here under the most favourable circumstances: the obscuration was so great that the disk could barely be discerned with the naked eye, and the copper colour usually seen was not noticed. Having watched the moon well into the umbra, my attention was diverted for a while, but, on looking again, at 9.35 G.M.T., I was surprised to see a portion of the north-east quadrant pretty strongly illuminated; my attention was again diverted, but on looking a second time at 10.35 G.M.T., I observed a portion of the south-east quadrant



illuminated in a somewhat similar manner. At both times the moon was well within the geometrical umbra. But the remarkable feature was that on both occasions the boundaries of the illuminated portions were, approximately, circular, and convex toward the axis of the umbra, indicating that the refracted solar rays producing these illuminations had crossed the axis of the shadow-cone previous to impinging on the lunar disk. The portions of the refracting annulus of the earth's atmosphere concerned in producing these effects were those superincumbent on the Southern Indian Ocean and the North Atlantic.

WENTWORTH ERCK

Shankill, Co. Dublin, November 4

### The Sky-Glows

IN using the word "corona" to designate the coloured glare which has accompanied the sun during the past year, I had no intention of employing it in its astronomical sense, but in its ordinary meteorological meaning—which "G. M. H." (*NATURE*, vol. xxx. p. 633) has overlooked—as referring to the coloured circles on cloud and haze frequently to be seen round the sun and moon, and classed by some observers with halos. By calling the circle now visible round the sun a "corona," I mean that in appearance and probable optical cause it is more like a meteorological corona than like a halo.

May I be allowed to point out a misprint in the first paragraph of my last letter (vol. xxx. p. 633), where it should read "unusual sky phenomena"—the word *universal* having been printed for *unusual*.

T. W. BACKHOUSE

Sunderland, November 8

AFTER sunset this evening there was a peculiar pink flush in the western sky here similar to that which attracted so much attention in England last year. Twenty-five minutes after the sun had gone down, the colour was so vivid as to be reflected from the snows of Mount Baker (10,700 feet), which is about seventy-five miles east of this place. Shortly afterwards it disappeared, but reappeared thirty-five minutes later, prolonging the twilight and making the stars look green, finally dying away very gradually. The weather for the past twelve days has been very wet, and to-night's is the first clear sunset in that time.